

**Sherryl Davis Kasper. *The Revival of Laissez-Faire in American Macroeconomic Theory*. Cheltenham, UK, and Northampton, MA, US: Edward Elgar. 2002. 177 pp. ISBN 1 84064 606 3. US\$75.00.**

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**John F. Henry\***

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Professor Kasper addresses the question as to whether the revival of a laissez-faire program was the consequence of external, social problems – the inflation and stagnation of the 1960s and 1970s; internal theoretical developments, including the creation of new analytic techniques; or a response to an ideological predisposition toward a non-interventionist laissez-faire agenda. If the latter, then ‘the analytic character of economics is called into question’ (p. 3).

Restricting her analysis to six major figures, Kasper subjects the work of Frank Knight, Henry Simons, Friedrich von Hayek, Milton Friedman, James Buchanan and Robert Lucas to a cursory examination to assess the work of each with regard to the main question above. Though this point is never emphasised, each of these stalwarts was (or is) associated with the University of Chicago.

The most interesting of this set, at least in my opinion, is Knight. Somewhat cautiously, Knight developed his case for laissez-faire on the basis of the theoretical implications associated with uncertainty. Though the ‘enterprise’ system was neither ‘necessary or inevitable’ (p. 19), and certainly had no pronounced ethical foundation, it was the most efficient system attainable and that which promoted the greatest amount of personal freedom – Knight’s standard by which to judge alternative economic frameworks.

Simons, who produced the 1934 tract, *A Positive Program for Laissez Faire*, developed the most unusual recommendation for laissez-faire. Building on the uncertainty theory of Knight, and recognising that capitalism had undergone evolutionary change, particularly in the growth of non-competitive industrial forms, but also committed to classical liberal ideology, Simons called for an activist state to enforce laissez-faire – seemingly a contradictory position.

Hayek, as is well known, based his position on the subjectivist individualism of the nineteenth-century Austrians and developed his strident defence of laissez-faire initially in the context of the 1930s wrangles over the business cycle and, more importantly, in his 1937 work on *Economics and Knowledge*, imperfect information. We see in Hayek, then, a similarity to Knight in establishing a theoretical base for laissez-faire.

Friedman broke with his Chicago mentors in undermining Knight’s uncertainty-based defence of laissez-faire with his expected utility hypothesis and with his positivist program that eliminated (at least superficially) the apriorist and deductive logic foundation of the older rationalisation. It should also be noted that Friedman issued his own version of Simons’s *Positive Program* in his much more strident, anti-interventionist *Capitalism and Freedom* (1962). This work may well be the definitive postwar statement promoting laissez-faire, eclipsing even Hayek’s *Road to Serfdom* in popular appeal.

While Friedman’s major work in economic theory, particularly his new form of monetarism, may appear to be value-free, Kasper demonstrates that Friedman himself has admitted to an ideological, value-laden presumption in favour

of classical liberalism and a rules-bound government with no or little discretionary power (p. 101).

Buchanan described himself as an anti-market 'libertarian socialist' in the early part of his life (p. 105), though his graduate training under Knight at Chicago quickly convinced him of the superiority of the market. Both his early work on public finance and his more famous labours in the realm of public choice theory combined the subjectivist individualism of the Austrians with the positivism of Friedman to generate his particular slant on laissez-faire. His most compelling statements on this program are set forth in his *The Limits of Liberty* (1975) and, with Richard Wagner, *Democracy in Deficit* (1977). Here, combining economic analysis with his theory of voting behaviour, Buchanan argues that the natural tendency of voters is to promote governments that will expand the provisioning of public goods, thus running deficits and spurring inflation (p. 118, ff.). Hence there must be well-established rules to counter natural tendencies based on voter perception and to constrain government involvement in the economy (p. 123, ff.).

Regarding Lucas, Kasper demonstrates the connection between the eventual development of the New Classical theory and the work of the other laissez-faire theorists examined here. The distinction of Lucas's work is that it is based on new (mathematical) techniques coupled to GE theory and is testable econometrically. In Lucas we see the final marriage of the micro theory of neoclassicism, based on the abstract rational agent, and macro theory. Indeed, macro disappears in substance, replaced by microfoundations. Keynesian (however defined) objections to laissez-faire and defence of some amount of government intervention fall by the wayside.

Following her evaluation of the six major figures, Kasper concludes that the issues of inflation and stagnation, coupled with gaps in Keynesian theory, certainly did provide a stimulus to the recrudescence of the laissez-faire program. But this, in itself, could not explain the rise and success of Lucas in particular. Why New Classical theory, after all? Why not a reemergence based on Knight?

While Knight did base his argument on the theoretical principle of uncertainty, and Simons drew on Knight in developing his case for laissez-faire, Hayek's argument stands or falls on the methodological individualism of the Austrians. While Friedman and Buchanan were not convinced of Knight's theoretical argument, they did imbibe the classical liberalism most pronounced in the work of Simons and Hayek. However, the positivist portion of their defence failed. Lucas, in developing the mathematical techniques associated with new classicism – techniques with which modern US graduate students are familiar – seemingly removed the 'taint of ideological commitment' from the laissez-faire program. However, Kasper notes that the arid, technique-driven argument of Lucas is certainly not free of ideological taint. Even the choice of tools is influenced 'on the ground floor' by ideology (p. 151). And Lucas, trained in the Chicago mode, 'internalized' the laissez-faire ideology uncritically.

At the same time, Kasper notes that none of these developments would have proved successful had not Knight, Simons, and Hayek established a 'community' within which laissez-faire economists could share ideas, prod each other, and find safe haven for their dissenting ideas. The Chicago economics department, the Mont Pelerin Society, and similar institutions, provided the framework within which a distinct minority could maintain their allegiance to classical liberalism and laissez-faire while the rest of the profession adopted an interventionist, 'mixed economy' mode of thinking.

How successful is Kasper in arguing her case? In my opinion, she remains too much at the surface of the investigation she undertakes. I accept that ideology is at the bottom of neoclassical theory in whatever form it takes. I also accept that ideology is significant in the development of *any* theoretical system. How, though, to prove that it is ideology that is driving the argument leading to pre-determined outcomes? This is a large question that requires a large answer, one based on a much fuller examination than is provided here.

I would also have liked a much more thorough treatment of Knight and Hayek in particular. Knight, especially, was a very complex thinker, and his tortuous ruminations surrounding ethical principles and guarded advocacy of laissez-faire deserve a much more elaborate statement than that given here. Greater attention to the secondary literature would have been welcome. Here, I just mention Robert Nelson's *Economics as Religion* (2001) that focuses on Chicago and the fundamental break between old Chicago (Knight) and new Chicago (Friedman, Becker, *et al.*) and which includes an exceptionally thoughtful chapter on Knight. The inclusion of such works would have made Kasper's argument more compelling.

I am also not convinced by the supposed relation between the problems associated with Keynesian theory (so-called) and the opening this allowed laissez-faire theory when the inflation and stagnation of the 1960s and 1970s struck. There has been a heated debate as to what exactly is meant by 'Keynesian' theory, but Kasper seems to accept the neoclassical synthesis version as representative of this framework rather than that proposed by Joan Robinson and other Post Keynesians. Was it Keynes who was responsible for the theoretical 'gaps', or the neoclassical 'Bastard' Keynesians? This is important, as the teaching of vulgar Keynesianism in the US (in particular) made it more difficult for students to arm themselves with a theoretical system that might have prevented (or slowed) the ascendancy of the laissez-faire program.

Lastly, I would like to have seen much more on the 'community' within which Knight, Hayek, Friedman, and other laissez-faire theorists worked. The Mont Pelerin Society is mentioned almost in passing, but this was clearly an influential *political* organisation that, along with other such organisations, had an enormous impact on governments, on students, on economics. Developing the argument found in Richard Cockett's *Thinking the Unthinkable* (1994), for example, would have been most beneficial in providing more background information as to the nature of this community and the heavy-hitters who were influenced by Hayek and others. Also, one should not overlook the fact that, in the 1950s, non-orthodox economists were largely silenced in the United States, generating an environment that gave free rein to those who espoused any anti-collectivist ideology. So, while one community flourished, another was effectively eliminated. Again, these developments affected the training of young economists who might have been better able to resist the blandishments of mere technique and, thus, the appeal of a Lucas.

But these criticisms may well call for a different book, certainly one that is much longer than that under review. And, at a price of US\$75 (or 42 cents per page!), maybe the longer, fuller account should have been expected.

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**Robin Hahnel. *ABCs of Political Economy: A Modern Approach*. London: Pluto Press. 2002. xv + 304 pp. ISBN 0 7453 1857 6 (hb), ISBN 0 7453 1858 4 (pb). £45.00 (hb), £14.99 (pb).**  
**Frank Stilwell. *Political Economy: the Contest of Economic Ideas*. South Melbourne: Oxford University Press. 2002. vii + 440 pp. ISBN 0 19 551458 0. \$A49.95.**

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**J.E. King\***

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Heated arguments have broken out at recent history of economic thought conferences in both Australia and the United States on the relationship between our discipline and heterodox economics, especially its institutionalist, Post Keynesian and radical-Marxian versions. Intellectual historians sympathetic to mainstream economic theory complain that their specialism has been hijacked by ratbags, while the dissidents reply that it was the neoclassicals who abandoned ship, often blowing open large holes in the hull as they did so in an attempt to sink it.

The appearance of two new and very different introductory textbooks on (radical) political economy may shed some light on this controversy, the history of ideas having featured prominently in an earlier generation of counter-texts, both successful and unsuccessful (see Hunt and Sherman 1972, and Robinson and Eatwell 1973, respectively, and, on the latter, King and Millmow 2003). Frank Stilwell's *Political Economy* is very much in this tradition, and takes the history of economic ideas very seriously indeed. The whole of part III, 'Back to basics' (pp. 61-89), consists of a highly readable introduction to classical political economy. It is followed by no less than six chapters (pp. 93-143) on Marxian economics. This is very well done: it is clear, undogmatic and very thorough. There are also brief accounts of the origins of neoclassical theory, with an emphasis on Marshall (pp. 148-52), and on the history of welfare economics, emphasising Pigou and Pareto (p. 199). Stilwell then stresses the history of thought dimension in his discussion of institutionalism, with an entire chapter on Veblen (pp. 199-205) and additional material on other writers in this tradition. His first chapter on macroeconomics presents the emergence of Keynesian theory as a challenge to classical macroeconomics (pp. 251-8), and the final chapter in this section outlines the monetarist counter-revolution and the Post Keynesian renaissance, again mainly in historical terms (pp. 288-99).

Thus the history of thought content of Stilwell's *Political Economy* goes well beyond the tokenistic 'great men in boxes' who are often found in mainstream texts, though there are plenty of them, too: in order of appearance, Smith, Mill, George, Marx, Engels, Marshall, Joan Robinson – the only woman in a box – Veblen, Myrdal, Schumpeter, Galbraith, Wheelwright, Keynes and Friedman. All this comes at a cost, both literally – it is a large and quite expensive volume – and otherwise. Some important issues are simply omitted, like the collapse of the Soviet economic system and the rise of the anti-globalisation movement. Others are handled rather superficially; the basic income-expenditure version of the Keynesian macroeconomic model should certainly have been set out at greater length. But *Political Economy* is very clearly the work of someone with respect for, and considerable knowledge of, the history of economic ideas.

Robin Hahnel's *ABCs of Political Economy* is a different book altogether. It is shorter, and smaller, than Stilwell's: you could read it in bed if you so chose. Its structure is much more conventional, with discussion of philosophical issues (chapters 1-2) followed by analysis of micro (chapters 3-5), macro (chapters 6-9) and policy alternatives (chapters 10-11). Its treatment of the history of economic thought is generally perfunctory and sometimes tendentious. Thus Adam Smith is presented as a dogmatic and unqualified neoliberal (pp. 80-4) and Keynes – in a passage almost calculated to make David Laidler foam at the mouth – as the creator of macroeconomics, apparently out of nothing (p. 128). Important questions are avoided throughout the book, from Hahnel's opening account of 'liberating theory' as an alternative to historical materialism in the absence of any serious summary of Marx's theory of history, through to the listing of his own heroes in the final chapter, where Sidney and Beatrice Webb are bracketed with William Morris as 'utopian socialists' (p. 281n8). Arguable, perhaps, but not argued here. Hahnel devotes the whole of chapter 3 to a simple but ingenious corn model (pp. 45-70), but makes no reference to Ricardo until p. 141, nor to Sraffa until p. 113. There is an extensive analysis of externalities and the consequent gaps between private and social benefits and costs (pp. 84-9, but no mention of Marshall or (until p. 272) Pigou, let alone the Coase theorem). The Okishio theorem is, however, invoked, in refutation of Marx's theory of the falling rate of profit, but Hahnel makes no real attempt to explain the theory that is being demolished (pp. 124-5). He provides a bare reference to John Nash (p. 210n3), very late in the day and only after many pages have been dedicated to game-theoretical models; unbelievably he ignores both the book and film versions of *A Beautiful Mind*, as if Hollywood lionises economists on a regular basis.

This is not to say that *ABCs of Political Economy* is without merit. In some ways, indeed, it is distinctly better than *Political Economy*, since Hahnel sets out both neoclassical and heterodox models in much greater depth than Stilwell; the contrast is especially evident in their respective macroeconomics chapters. A diligent reader will come away from Hahnel's book knowing a great deal about the structure of some mainstream and much non-mainstream economic thought, but almost entirely ignorant of their histories. Political economy, it seems, may *use* but does not *need* the history of economic thought. It remains to be seen whether the converse proposition is also true.

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**Robert Leeson. *The Eclipse of Keynesianism. The Political Economy of the Chicago Counter-Revolution*. Basingstoke and New York: Palgrave. 2000. vii + 182 pp. ISBN 0 333 73045 3. \$A157.00.**

**J.E. King\***

The 2003 ‘better late than never’ award goes to Palgrave, whose review copy of Robert Leeson’s book arrived a full three years after publication. It is a companion to his volume on A.W. Phillips (Leeson 2000) and contains six chapters, four of them previously published, on the triumph of Chicago macroeconomics after 1968. Chapter 2, on the Keynes-Tinbergen-Friedman-Phillips critique of ‘Keynesian’ macroeconometrics, originally appeared in *History of Political Economy* (in Spring 1998, though you would not learn this from Leeson’s preface). Chapter 4, on the rise of the natural rate of unemployment model, and chapter 6, on ‘language and inflation’, were first published in *History of Economics Review* (both in issue 25, the 1996 special volume on Keynes), while the brief chapter 5, also on the natural rate, is from the *Cambridge Journal of Economics*. Neither chapter 3, on ‘the Chicago counter-revolution and the sociology of economic knowledge’, nor (apparently) chapter 7, on ‘Friedman and the Walrasian equations of the natural-rate counter-revolution’, has been published before. Chapter 1 is a brief introduction. The book runs to 130 pages of text, plus notes (18 pages), the expected comprehensive bibliography (27 pages), and an index.

Leeson’s central theme is taken from George Stigler, who played Mr. Micro to Milton Friedman’s Mr. Macro at Chicago: the economist as huckster (p. 52). According to the *Shorter Oxford Dictionary*, the term refers to a retailer or publicity agent (the third definition is ‘a mercenary person ready to make a profit out of anything’). As Leeson shows, Friedman must be rated as one of the greatest hucksters in the history of economic thought and policy, marketing Chicago macroeconomics on the basis of its supposed theoretical and empirical superiority over the Keynesian synthesis. Somehow he kept a straight face while asserting the merits of the natural rate of unemployment that was ‘ground out by the Walrasian equations of general equilibrium’ and corroborated in practice by rigorous and sustained econometric testing, even though: (i) he shared Keynes’s suspicion of macroeconomic modelling, and for very similar reasons; (ii) he was, again like Keynes, a confirmed Marshallian who had very little time for Walras; and (iii) his own predictive failures rivalled those of the neoclassical Keynesians he derided. (Leeson cites David Laidler and Patrick Minford as two disillusioned ex-believers in his forecasts.) Friedman’s campaign involved just a bit more than rhetoric, though his lucid and engaging prose style certainly helped to convince the waverers. With apologies to Oliver Williamson, I am inclined to describe it as ‘rhetoric with guile’. Leeson denies ‘that Stigler and Friedman were engaged in some sort of conspiracy’ (p. 47), possibly because their behaviour was too bare-faced to merit the term. It was undeniably opportunistic, and it worked like a charm.

This poses two questions. First, why was mainstream Keynesian macroeconomics so vulnerable at the end of the 1960s to this sort of

hucksterdom ('derog.': *Shorter Oxford Dictionary*)? Leeson points to the intellectual and policy weaknesses of the Keynesian synthesis, in particular the Phillips Curve approach to inflation and anti-inflation policy. He might have noted that similar criticisms had already come from the intellectual left: Sidney Weintraub (1961) was the first theorist of stagflation, if not the first accelerationist, while Joan Robinson – who flits through the book like an unwelcome ghost – soon became a remorseless critic of 'pre-Keynesian economics after Keynes' (Robinson 1964). Second, why did Chicago macroeconomics prove so resilient (unlike Chicago microeconomics, which is a very dead dog indeed)? As Leeson observes, 'the Monetarist natural-rate model remains at the core of applied macroeconomics' (p. 130) – in spite of the shaky theoretical foundations, the failed predictions, the enormous human cost of monetarism in practice. But he never really explains why. For an account of the success of monetarism as conservative ideology it is necessary to look elsewhere (Bhaduri and Steindl 1983 is a good place to start).

This is a provocative and intelligent book. It could have done with careful editing, however. Leeson might have deleted the inflated opening claim that 'This book examines the intellectual revolution that separates the last quarter of the twentieth century from the preceding four decades' (p. 1), as if economics really *was* everything, and he really should have corrected the unfortunate reference to Irving Fischer (p. 133 – the father of Stanley, perhaps?). There is too much repetition, in particular of tasty quotes: Robinson on Keynes on the long run (pp. 78 and 126), Max Weber on toothbrushes (pp. 43 and 98), and Harry Johnson on the petering out of monetarism (pp. 30, 89 and 128). At \$1.20 per page of text it can hardly be described as an essential purchase, especially as so much of it has already appeared in print. But it does make for absorbing, if also rather depressing, reading.

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**William O. Coleman. *Economics and its Enemies: Two Centuries of Anti-Economics*. Basingstoke: Palgrave Macmillan. 2002. x + 313 pp. ISBN 0333790014. £60.00.**

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**Frederic S. Lee**

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I started reading this book on the way to a heterodox economics workshop on methodology. The further I got into it the more it became clear that I was, according to William O. Coleman, an enemy of economics and the workshop I was going to was really a terrorist workshop where I would be training young impressionable economists to be anti-economists. *Economics and its Enemies* is a book that is designed not to defend economics from its detractors but to utterly vanquish them.

The book can easily be summarised. Coleman begins *Economics and its Enemies* with identifying and delineating what constitutes an anti-economist. Then in chapters 2-10, he examines the political, philosophical, moral, religious, and humanistic arguments against economics. Each chapter is devoted to a specific anti-economics argument, and the anti-economists who made them are identified; and each chapter is written in a highly critical and polemic style (which I very much liked) where the anti-economists are vanquished. Indicative of his style are the titles Coleman chose for his chapters, such as 'The Wretched Procurers of Sedition' and 'The Apostles of the Rich'. In chapters 10-13 of the book, he explains why the anti-economists have made so little headway against economics; and in the book's final chapter, 'The Not-so-Puzzling Failure of Anti-Economics', he argues that it will never succeed in its attack on economics.

There are, however, a number of problems with the book. The first and foremost is Coleman's definition and delineation of an anti-economist. He starts by defining an economist as a person with specialist knowledge of economics. Coleman then defines economics as 'that stream of thought which stretches from the eighteenth century to the present day, and which embraces Smith, Ricardo, Mill, Walras, Hicks and living persons. This continuity of thought could be called the "Mainstream"' (p. 8). This clearly implies that economics to Coleman is neoclassical economic theory. Hence an anti-economist is simply someone who rejects neoclassical theory in total or many of its components such as maximisation, competition, allocation of scarce resources among competing ends, market equilibrium, economic laws, deductive methodology, constraints and costs, and the normative-positive distinction. Moreover, 'an anti-economist is whoever sees economics as a bane' (p. 7), and this person would want to do away with teaching it, abolish the Swedish (Nobel) Prize in economics, and remove economics from any impact on public policy. Clearly there are many people past and present who fit this description, but Coleman aims many of his attacks at those whose knowledge of 'mainstream' theory is minimal and hence could not really be considered as economists. For example, I would not have considered Samuel Coleridge, Thomas Carlyle, or William Wordsworth as economists (although they did make anti-economics comments). In contrast, those anti-economists who reject mainstream or neoclassical theory because it is theoretically incoherent and hence consider that markets without at least some regulation could generate baneful outcomes are

dismissed in a footnote (pp. 255-6). By choosing easy targets, Coleman has substantially weakened his own case, which for this reviewer is unfortunate.

There are three other problems with the book. First, Coleman tries to cover too much and hence does not provide the in-depth discussion that one would expect when, for example, claiming that all of American Institutionalism is anti-economics or that economists are not hired ideologues of capitalism. Many chapters, such as those on the moral economy or economic method, deserve books to themselves. Consequently, *Economics and its Enemies* takes on the appearance of being a survey that just skims the surface. While the reader constantly begs for more details, he (in my case) also appreciates Coleman's efforts to bring the various disparate arguments and anti-economists together under a single roof.

Secondly, Coleman makes numerous 'absolute' statements that are absurd if not just plain wrong. For example, he contends that economics '...has not justified, legitimated and apologized for [the] ruling order' (p. 52). Such a statement is contentious at best, but apparently Coleman is not familiar with the presentation of neoclassical theory in American introductory economics textbooks. In another instance, he claims that '...the market is not a bane. It is a highly useful contrivance....Yet no one, we contend, will ever discover that the market despoils' (p. 232). In light of the Great Depression and markets for human bodies, health care, and education, this is clearly a questionable statement.

The final problem is that Coleman presents an unbalanced view of the shortcomings of anti-economists and their arguments. The clearest example of this is his discussion in chapter 13 of the homology between certain forms of anti-economics and anti-Semitism. I would not deny Coleman's thesis, but there are also instances when neoclassical economists merge racism with theory, such as the case of Thomas Schelling's 'checkerboard' model explanation of racial segregation (Schelling 1978). In real life, racial segregation in American cities, such as Kansas City, was a result of deliberate housing policies by racist home construction companies and realtors and not the result of unintended consequences of individual actions. Although made void by Supreme Court decisions, these policies still have an impact on residential housing patterns, especially when banks and other financial institutions today engage in red-lining. Schelling-type explanations of segregation trivialise racism and hence endorse it. Does this mean that neoclassical economists are racists? Perhaps. Does this have anything to do with neoclassical theory? Maybe, maybe not, but Coleman ignores this damaging evidence when criticising the anti-economists for anti-Semitism and pro-slavery attitudes.

Do these shortcomings mean that *Economics and its Enemies* should be summarily dismissed? I think not. I read or more precisely yelled at the book and argued with each page. In the process I did learn much about anti-economists that I did not know. Coleman's *Economics and its Enemies* provided me with a valuable learning experience and there are not many authors and books that can do that.

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<p><b>Ulrich Witt. <i>The Evolving Economy: Essays on the Evolutionary Approach to Economics</i>. Cheltenham: Edward Elgar. 2003. 416 pp. ISBN 1 840 64748 5. £75.00.</b></p>
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Ulrich Witt is Professor of Economics and Director, Max Planck Institute for Research into Economic Systems, Jena, Germany. *The Evolving Economy* includes his published papers from the 1980s until recently and is a volume of 20 essays divided into five parts, with each part revealing a theme. The first part consists of a new, introductory paper. Part two consists of five essays that address 'Evolutionary Concepts and Methodology'; part three (four essays) addresses 'The Darwinian Perspective and the Continuity Hypothesis'; part four (six essays) investigates 'Evolution in the Context of New Institutional Economics and Public Choice', and part five (four essays) focuses on 'The Evolutionary Approach and the Austrian School of Economics'. The following review highlights issues dealt with by Witt from selected papers, which are presented in each of the stated sections.

Witt argues that economic transformations lead to constant change. Are there common factors behind these transformations? If so, how does one identify them? What concepts and tools need to be developed, and how? Witt acknowledges the need for an heuristic process to develop a method to identify evolution in the economy, a difficult task but necessary if one does not wish to accept the variety of change as simply the haphazard product of history. Of course, the very nature of evolution implies that he has set himself a challenge: to identify causality within the context of random evolution processes in the economy.

This is an interdisciplinary task. Witt notes that his investigation of, and subsequent writing for, unconnected economics groups meant that his writings were unknown outside each of these groups. This book of his writings collects these together in one volume. They reveal an attempt to bring together his 'grappling' with conceptual questions in an effort to make sense of evolution in the economy. It is a rich collection and one that clearly reveals depth of investigation, and long, sagacious consideration.

The introductory essay addresses conceptual and methodological problems in the development of the evolution of the economy. 'Historically, change...has governed the human economy...' (p. 3). The cause is endogenous, according to Witt, and not exogenous, as in the dominant paradigm (p. 4). How is change conceived? The conception is that of evolution by self-organising and self-augmenting processes both coordinating to, and diverging from, equilibria that occur in time, in 'living systems' (p. 5). The endogenous self-transformation over time of a living system includes the emergence and dissemination of 'novelty' (p. 13).

The book's central assumption is the 'ontological continuity' in the way evolution extends into the process of long-term economic change (p. 28). However, '...conceptions and explanations must...be found which do not rest on (potentially fallacious) Darwinian analogies' (p. 18). Indeed, Witt is clear that a theory needs to show how change is initiated, where it occurs in the economy, the way it occurs, why it appears unstoppable and the implications for the world at large (p. 27).

Evolutionary thought has a traditional place in economics, he attests, though at the theoretical fringe; it has played an inspirational role in an evolutionary approach to economics but it needs development and clarification.

'Evolution can be defined as the self-transformation of an observed system over time' (p. 48). Witt suggests that evolutionary theories must be dynamic, address irreversible processes and incorporate novelty as a source of self-transformation (p. 37). This implies endogenous change, but how is it produced? It is generated by novelty. Novelty '...is the outcome of human creativity' and, if novelty is adopted, the action is 'called an innovation... [but]...by its very nature nobody can positively anticipate its meaning and implication' (p. 48). One consequence of this is that evolutionary theory is seriously constrained in its attempt to predict (p. 48).

In the paper, 'Coordination of Individual Economic Activities as an Evolving Process of Self-Organization,' Witt attempts to develop a theory of the market coordination process by spontaneous internal and external forces, which control divergence. Why? The 'fictitious' assumptions of neoclassicism lead to a 'fictitious' state of coordination in equilibrium. The alternative is that individuals continuously coordinate to find better outcomes, even if perfect coordination is not achieved. He examines the economic coordination problem from a perspective wider than that of neoclassicism and includes factors such as uncertainty, fallibility, inventiveness, and forces that affect the individual and market.

Witt considers economics from a Darwinian perspective (or bionomics) in his essay 'Bionomics as Economics from a Darwinian Perspective,' and offers a number of suggestions in which this view might contribute as an alternative: as a paradigm shift; as an incentive for economists to borrow from the success of Darwinian theory; and as a point of departure. Why the latter? Because Darwinian theory, at this stage, is only indirectly relevant to economic theory.

One wonders why Witt needs to introduce natural selection concepts into economics – '...the human economy is...hardly explicable in terms of the theory of natural selection' (p. 3) – when the concepts of Newtonian mechanics have created the poor 'optimisation-cum-equilibrium' explanation currently dominant. That said, while I am opposed to Darwinian theory and do not support importing its presuppositions into economics – "what are the genes?" or...[what is] the economic analogue to the species?' (p. 143), and what are the economic analogues for the source and driver of endogenous mutation and selection, genetic drift, gene flow and adaptation? – Witt's fruitful endeavours reveal significant insights, including transition laws (how the system proceeds from one state to another), the capacity of a system to generate novelty, motivations and rate of search for novelty (such as curiosity, and dissatisfaction or fear) (p. 146), and a common set of 'inherited preferences' that provide benefits revealed in long-term economic evolution.

In the paper, 'Self-Organization and Economics – What is New?' Witt considers the evolutionary aspect of the theory of self-organisation as discussed in the sciences. Does self-organisation play a role in economics, and do self-organisation concepts in the sciences provide insights for economic theory? Witt conjectures that economic production may be considered an extension of nature's self-organisation, and insights from nature may improve one's understanding of how long-term economic development might occur. However, this investigation is incomplete and largely unclear.

‘It is intelligent human action, imagination, and a growing knowledge that support the process [of self-organisation]’ (p. 212). The competing, cooperating interactions of multitudinous agents foster a self-organised collective outcome that includes the features of self-regulation and self-augmentation. This latter feature, he explains, is the result of individuals’ motivation ‘...to search for, and experiment with, new economic knowledge and to implement it in innovative action...’ (p. 214). It is effectual because it induces a transition to better outcomes.

In ‘The Endogenous Public Choice Theorist,’ Witt addresses Buchanan’s ‘contractarian paradigm’ (i.e., voluntary agreement requiring consent by all on a proposed change if one’s welfare is not to be reduced) and Hayek’s ‘constructivist rationalism’ (i.e., that neither rules nor spontaneous order are an outcome of intentional planning). He addresses the problem of ‘self reference’ (i.e., the outside intervention of the normative political economist, which is largely an implicit contradiction in both Buchanan and Hayek) by endogenising the effect the political economist might have on the evolution of society (p. 240). How does he accomplish this? Competitive processes occur between suppliers of information to evolve active (useful) knowledge and, therefore, a comparative advantage will probably occur in communication skills and creativity. This then becomes part of the role of the normative political economist (p. 242) – one of propagandist. His motivation: ‘If successful, they usually do get substantial rewards in terms of honor, power, and wealth’ (p. 244).

In the essay, ‘Subjectivism in Economics – A Suggested Reorientation,’ Witt considers that if consistent the *a priori* subjectivism (i.e., praxeology, as propounded by Ludwig von Mises) of the ‘Austro-American’ school would reduce economics to a ‘sterile logic’ incapable of explaining empirically observed economic behaviour (p. 331). He proposes a return to the psychological foundations of the early Austrian writers to explain individual behaviour, utilising research into neighbouring disciplines, such as psychology, without interfering with ‘the subjectivism problem’ (p. 331).

In the paper ‘Endogenous Change – Causes and Contingencies’ Witt considers changes in ‘action knowledge’ and its impact on the economy in the light of the dominant view’s explanation of economic change, that is, exogenous disturbances and shocks. This limited explanation is contrasted with an explanation of change as that caused by changing knowledge and creative, individualistic activity. Thus this cause of change is endogenous and a result of ‘individual learning and the pursuit of novelty.’ (p. 364). By its nature novelty implies an ‘irreducible indeterminacy,’ so that the course of change cannot be determined (p. 365).

In the essay, ‘Turning Austrian Economics into an Evolutionary Theory,’ Witt considers Schumpeter and Hayek as two of the originators of evolutionary thinking in economics, and claims that the evolutionary approach shares process and change with the Austrians but that subjectivism has been ignored. This latter problem he attributes to Schumpeter, while, at the other extreme, he rejects Mises’ aprioristic approach because it conflicts with the nature of the evolutionary approach, in particular how ‘novelty emerges and disseminates in the economy’ (p. 373). He then goes on to discuss a fallibilistic and empirically oriented method, and considers possible cross-fertilisation between Austrian and evolutionary economics.

This is a delightful collection of Witt’s essays. One sees a fertile mind on the task at each turn, not willing to accept the status quo but redressing the

significant contribution of scholars' work by the inclusion of evolutionary concepts. There ought to be something for everyone here, with implicit challenges throughout to laterally expand and extend the parameters of economic science in new and exciting directions. Witt's human agent is no maximiser, but he does see the issues with each paradigm, and sets out to reconsider each and, in some cases, tamper with the undergirding edifices. Certain aspects of evolutionary economics are, however, still in their nascent stage and others are a work-in-progress. There is no doubt that Witt has applied his resolve and energy to evolutionary economics, and this volume is a testament to his endeavour.

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<p><b>Jürgen Backhaus (ed.). <i>On the Frontiers of the Modern Theory of Value: Essays on Attilio da Empoli (1904-1948)</i>. Erfurt Studies in Public Finance and Fiscal Sociology, Number 3. 2002. 175 pp, privately circulated. ISSN 1619-8514.</b></p>
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**Michael McLure\***

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The collection of essays included in *On the Frontiers of the Modern Theory of Value: Essays on Attilio da Empoli (1904-1948)* – a pre-print of the *Journal of Economic Studies* 28 (4-5), 2001 – provides a sympathetic appraisal of Attilio da Empoli's approach to public finance and economic equilibrium. All are in English. Da Empoli is most well known in his native Italy for generalising the prevailing partial analysis of tax incidence by introducing the concept of 'oblique tax shifting' in his 1926 *Teoria dell' Incidenza delle Imposte*. Among other things, this work is important for analysing 'forward' oblique tax shifting, or the incidence of a tax on goods on consumers of non-taxed goods, and 'backward' oblique tax shifting, or the incidence of a goods tax on producers of non-taxed goods as a result of changes in the pattern of consumption. Perhaps more importantly, da Empoli's second book for 1926, entitled *Riflessioni sull'Equilibrio Economico*, provided the economic context within which his tax theory should be interpreted. Fortunately for English-speaking scholars this book was revised, translated and enhanced when da Empoli was a Rockefeller Foundation Fellow, and published in the United States in 1931 as the *Theory of Economic Equilibrium: A Study of Marginal and Ultramarginal Phenomena*. This original work presented the economy as a system where rents are standard because the law of one price is rejected in favour of determining equilibrium price ranges. The dynamic elements of production were accommodated within pure economics by supplementing orthodox 'marginalism' with an additional focus on 'ultramarginal' production (potential minimum additional product that would be produced if production were to be increased) and 'ultramarginal' utility (potential decrease in marginal utility if the ultramarginal product were to be produced). Within this system, marginal phenomena establish

the lower bounds of price ranges while the upper bounds are determined by ultramarginal costs (which vary between enterprises) and ultramarginal utility.

*On the Frontiers of the Modern Theory of Value* attempts to highlight da Empoli's original equilibrium analysis and approach to public finance, place his approach to equilibrium in the context of contemporaneous English-language developments, and establish da Empoli's place in the history of Italian economic thought. The editor of the collection, Jürgen Backhaus, also uses the Introduction to stress the current relevance of much of da Empoli's work.

The collection commences with a fine essay on 'Ultramarginal Cost in da Empoli's Work' by Massimo di Matteo and Luigi Luini. It clarifies that da Empoli refused to treat production on a marginal basis alone because he regarded production as a discontinuous process, and demonstrates that discontinuity in production provides the basis for da Empoli's presentation of 'rational' competition as a strategic process in which entrepreneurs make production decisions in light of their own actual and potential costs as well as production decisions by their competitors.

Perhaps the issues of most interest to historians concern the English-language reception accorded to the *Theory of Economic Equilibrium* and the scientific merits of this work relative to related developments in equilibrium theory. In this regard, da Empoli's focus on discontinuity and ultramarginal magnitudes represented an objection to perfect competition theory, raising points of comparison with Joan Robinson's imperfect competition and Edward Chamberlin's monopolistic competition. Furthermore, as da Empoli's rational competition presents production as a rivalrous process, points of comparison emerge with studies in the tradition of Heinrich von Stackelberg. These issues are addressed in a number of papers, the best of which are Peter Senn's 'Attilio da Empoli's English Language Reception', Jan Horst Keppler's 'Attilio da Empoli's Contribution to Monopolistic Competition Theory', Richard Wagner's 'Competition as a Rivalrous Process' and Gerrit Meijer's 'Da Empoli's Theory of Equilibrium in Retrospect'. Apart from Joan Robinson's largely critical review of the *Theory of Economic Equilibrium*, the book received little or no attention in the English-speaking world. Possible explanations for this range from the monograph having no enduring value (generally rejected), to being poorly organised and difficult to follow (conditionally accepted), and to reflecting original ideas that were too novel to be assimilated (generally accepted). Perhaps Richard Wagner sums it up best when he suggests that, as da Empoli considered competition as a verb at a time when leading English-speaking scholars were considering it as an adjective, his original ideas were unable to inspire the complementary studies needed for an international research program to develop.

Da Empoli's intellectual isolation may also explain why his equilibrium theory was not received any better in his native Italy. As Antonio Fusco notes in 'An Old Dispute', Umberto Ricci was fiercely critical of the notion of ultramarginal phenomena. In contrast, Domenicantonio Fausto's 'Issues in Public Finance Theory in Attilio da Empoli's Scientific Work' reveals that da Empoli's public finance theory was favourably considered in Italy by the likes of Achille Loria and Benvenuto Griziotti and that his debate with Mauro Fasiani, on the circumstances when tax incidence can be considered in isolation from public expenditures, was insightful.

Historians of economic thought will find this collection rewarding for its rediscovery of an approach to value theory and equilibrium that failed to have any

influence on the development of economics but which nevertheless anticipated developments in economic theory. Scholars with an interest in public finance and modern fiscal sociology may also benefit from considering Backhaus's argument that da Empoli's contributions represent an essentially Schumpeterian critique of the Stackelberg-Robinson-Chamberlin orthodoxy which has current relevance for theories of taxation and regulation. However, this is not a faultless work. Its two main shortcomings concern the repetition of similar themes in many essays and an unevenness in the quality of the essays (the best contributions have generally been highlighted in the course of this review).

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**Randall E. Parker. *Reflections on the Great Depression*. Cheltenham, UK, and Northampton, MA, USA: Edward Elgar. 2002. xii + 230 pp. ISBN 1 84064 745 0. £55.00.**

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**Alex Millmow\***

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One of the good things about becoming a star in the galaxy of top economists is that you have a more than average chance of doing well in the longevity stakes. Maybe we should advertise the fact. You will also still be in demand. One of the bad things is that in your autumnal years you can be pestered for dispensing pearls of wisdom to gushing young tyros. Parker, an academic from East Carolina University, fortunately is not in that class and, on balance, has done a public service in recording some of the last public words of five economists in this volume of interviews. The late and great include Charles Kindleberger (2003), James Tobin (2002), Moses Abramovitz (2000), Albert Hart (1997), Wassily Leontief (1999) and Herbert Stein (1999). The others in the complement Parker interviewed were those two titans of twentieth-century mainstream American economics, Paul Samuelson and Milton Friedman, along with Anna Schwartz, Morris Adelman and Victor Zarnowitz. While there are no Australian or European voices among them, some of the ensemble do (or did), in fact, hail from the Continent (Kindleberger and Zarnowitz). Apparently Parker had Martin Bronfenbrenner and Gottfried Haberler pencilled in for a chat but the grim reaper got there first. Since the prime topic for reflection and the grand theme of the book is about the Depression, J.K. Galbraith's omission, even from the index, *dramatis personae* and bibliography, seems puzzling.

Economic policymakers, like old generals, are in their heads always fighting the last war, and that was over the great inflation of the 1970s and 1980s, not the Great Depression. This book reminds us through the eyes of our elders just how cosmic the latter really was. All of the players stress that the Great Depression had a formative and resonating influence in their lives, not least on their choice of

economics as a vocation. Not that any of them wish for a recurrence of it. The Great Depression, as Ben Bernanke notes in the foreword, was ‘the holy-grail of macroeconomics’ in attracting passionate young scholars to the discipline to unravel what went wrong. In that vein, perhaps we need another economic calamity to attract young minds to economics, and, even better, the history of economic thought. Macroeconomics today is something of an academic backwater, although it hogs the front page of the newspaper. All the controversy has gone out of the subject and the public, equally, is likely to regard the term ‘Great Depression’ as evidence of something gone badly wrong with the community’s mental wellbeing. Before getting down to business Parker provides an overview of the slump, drawing out the debate on the leading hypotheses – monetary and non-monetary – behind the episode. Again, exploration of that still contentious debate is confined to the American economic experience. Parker unashamedly subscribes to the monetary hypothesis of Friedman and Schwartz (p. 41) and believes that current macroeconomic theory and policy strays very little from Friedman’s 1968 American Economic Association address (p. 118). That predisposition comes through, rather irritatingly, in his line of questioning of his subjects. His interviews with Friedman and Schwartz are self-indulgent, not only in length but also in his gushing approval of their words. In contrast, Parker’s interview with Tobin, the most clearly Keynesian of the gathering, is conducted without much warmth.

The most engaging part of this volume is, of course, the interviews with the exemplars of the American economic establishment. Kindleberger likened the process of being interviewed about past exploits to a scene from a J.M. Barrie play where retired generals are revealed polishing their medals. And there are certainly some medal-holders in this gathering. There is also plenty of tittle-tattle. The economists were asked a series of questions, ranging from the causes of and lessons from the slump to their careers, mentors, experiences and responses to Keynes. Their views are also solicited upon the low-lying terrain of twenty-first-century macroeconomics. Here some admit to not keeping up with the mapping, but all still have their economic compasses still working. The 64 trillion-dollar question of whether we could have a replay of 1930 is answered in the negative, except for Albert Hart, who does not regard the recurrence of ‘serious depression as impossible’. Modern macroeconomic management, as created by Keynes (1936) and Friedman (1968), will prevent it from happening again. Reference is made by some, however, to Japan’s slump and the re-appearance of the paradox of thrift and that even rarer bird, the liquidity trap. Also most of the respondents expressed ominous concern about the skewing of America’s income and wealth distribution.

There are some interesting revelations in the interviews. For instance, the Chicago school, like their counterparts in England, put out a memorandum in favour of deficit spending in 1932. Friedman admits that the New Deal was a godsend in terms of giving economists jobs and influence within the federal bureaucracy. However he stridently upholds the view that the great contraction was caused by the government mismanaging the money supply, not by the market system *per se*. He believes this message still has not been learned. This was due to the prevailing sociology of the economic profession in the postwar world, which made it difficult for monetarism to take root and prosper. Paul Samuelson reminisces a little on the triumphant arrival of Keynes, though he agrees that the behaviour of the Federal Reserve Board did not help matters. He makes the telling point, though, that tampering with the monetary base to avert the Depression was beyond the ken of economists at the time. He suggests that the Great Depression

was a one-off, caused by a concatenation of quasi-independent factors. Some of Parker's respondents also take the opportunity to trot out their own pet theories regarding what caused the calamity. Everyone agrees that what ended the slump was the war. There was no Keynesian Revolution in policy-making before the war. Even Keynes acknowledged as late as 1939 that the resistance to his new theoretical framework meant that the impact of the *General Theory* would not prove immediate. Only under an authoritarian regime could his grand experiment come to fruition. 'A change in mental atmosphere was a necessary condition for the bold experiment in achieving full employment by the methods I advocate' (cited by Skidelsky 1992, p. 619). The war provided those circumstances.

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**Allen Oakley. *Reconstructing Economic Theory: The Problem of Human Agency*. Cheltenham, UK, and Northampton, MA, US: Edward Elgar. 2002. xi + 234 pp. ISBN 1 84064 133. US\$110.**

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**Stephen Parsons\***

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The impetus behind this book by Allen Oakley lies in the perceived failure of mainstream economic theory to provide a methodology capable of appreciating how substantive economic phenomena are themselves products of human agency. This failure is traced to the unrealistic characterisation of human agency prevalent in mainstream economics. As the title of the book indicates, the intention is not primarily to provide a critique of mainstream economic theory, but to reconstruct the foundations of economics in the form of a 'humanistic social science'. This entails replacing the mainstream *homo oeconomicus*, where the assumption is that individuals know all relevant features of their situation, with a view of economic agents as subject to uncertainty and ignorance and embedded in situations which both constrain and enable actions.

Apart from an initial genuflection to transcendental realism, which unfortunately appears to have become obligatory in 'alternative' texts, the main authors that Oakley discusses in developing his alternative perspective on human agency are Schutz, Popper, Shackle, Simon and, to a lesser extent, Giddens. Although most economists will be at least acquainted with the ideas of Popper, Shackle, and Simon, the works of Schutz and Giddens are probably less familiar.

Alfred Schutz (1899-1959) was born in Vienna and studied under von Mises, although his writings are primarily influenced by the work of Husserl and Weber. Schutz argued that the social world was already symbolically prestructured by the individuals who comprised it. Individuals interpret their 'lifeworld' situation, and hence the theories produced by the social investigator must connect up with the everyday understanding of the individuals involved. Anthony Giddens is one of a number of contemporary social theorists who have attempted to resolve the action/structure problem in social theory. Typically, individuals have been regarded as either completely autonomous agents or as passive 'products' of social structures and forces. Giddens argues that structures do not merely constrain, but also enable actions, and actions can both reproduce and transform these structures.

Oakley investigates these authors in turn in order to tease out the strengths and limitations of their ideas in developing an alternative view of human agency. These investigations are essentially self-standing, which reveals both the strengths and weakness of the book.

On the one hand this approach allows for an in-depth investigation of each author's ideas in their own right, independently of any need for comparisons. Schutz is praised for his querying of mainstream economic assumptions and developing an alternative 'rich and detailed understanding of the subjective and intersubjective nature of human agency' (p. 72), although also criticised for not adequately exploring the relevant limitations on agents' knowledge acquisition, particularly those raised by the question of time. In contrast Popper displayed an almost fawning admiration for economic theory, which, as Oakley keenly points out, is somewhat incongruous with his own methodological stance. A theorist who advocates the practice of 'falsification' and rejects an instrumentalist view of theory construction may have done well to pause for thought and wonder why the practitioners of the economic theory he so admired were more than happy to embrace this very instrumentalist perspective. Nevertheless, Popper's 'situational analysis' does draw attention to the significance of viewing human agents as embedded in situations and possessing limited intellectual capacities.

On turning to Shackle, Oakley recognises that the importance of intersubjectivity stressed by Schutz, and of situational analysis as developed by Popper, point to two areas unfortunately neglected by Shackle. Nevertheless, Shackle is praised for his emphasis on time and uncertainty, the creativity of human action, and the significance of imagination and expectations in human action. Regarding Simon, Oakley praises his emphasis on cognitive limitations, individual choice, and awareness that this is limited, although not determined, by the roles relevant to the individual's situation. However, Oakley is suspicious that Simon remains rather too close to the mainstream view, especially when appearing to accept that problems raised by uncertainty can be severely reduced, if not completely overcome, by further knowledge acquisition.

These discussions are all interesting and informative. This is especially true in the case of the investigations into Schutz and Shackle, which are more extensive than those into Popper and Simon. However, on the other hand, the disadvantage of this approach is that it diminishes the possibilities of either systematically comparing or attempting to synthesise the different perspectives of the authors discussed. Given that Oakley declines to explore these possibilities in

the chapters devoted to the individual authors themselves, it might be anticipated that the conclusion of the book will address the relevant issues.

However, after a brief reiteration of the investigations carried out earlier, the final chapter states that 'it is an unfortunate fact that ... the social and economic philosophers we have selected for inclusion here chose to make only very limited contributions to these essential themes of situational analysis' (pp. 200-1). There follows a discussion of some of the ideas of the social theorist Giddens, who, until now, had warranted only occasional mention.

Oakley's apparent position here immediately raises a number of questions. Firstly, if the 'essential themes' of situational analysis are important in understanding human agency, yet the authors under discussion have only made limited contributions to these themes, why have these authors been discussed in the first place? It would seem more pertinent to discuss authors who do address, in some form, these 'essential themes', rather than those who do not. Secondly, if Giddens's social theory does encapsulate the way forward regarding these themes, why is his work not given more prominence, and this earlier on in the book? Thirdly, is there a suspicion that, in the final analysis, the author implicitly recognises that any attempt to synthesise the different perspectives discussed is simply too difficult a task?

An example might serve to illuminate this suspicion. The discussion of Schutz includes one section entitled 'self-consciousness and time' and another entitled 'choosing and acting in time'. Unsurprisingly, given Shackle's concerns, the discussion of Shackle contains numerous references to his ideas on time, including the comment that 'it was for its lack of any time consciousness ... that Shackle attacked the orthodox "model of man" used in neoclassical economics' (p. 114). In the final chapter Oakley offers his own view that human agents are 'embroiled in a perpetual stream of consciousness that only moves forward in time' (pp. 193-4).

Time is thus regarded as an important phenomenon by the two authors discussed in most detail, and by Oakley himself. However, are these different ideas on time compatible? How does Oakley's position relate to those of Schutz and Shackle? Do Schutz, taking his lead from Husserl, and Shackle, taking his lead from Descartes, mean the same thing by 'time consciousness'? One gets the sense of interesting issues which would warrant further investigation.

In conclusion, this is a good book for anyone wanting to get to grips with the ideas of Schutz and Shackle in particular, but also of Popper and Simon, and their respective criticisms of *homo oeconomicus*. However, there is a sense of two books struggling within the confines of a single volume, to the detriment of both. One book is primarily concerned with contrasting, comparing and, if possible, synthesising the views of the authors discussed. The other is primarily concerned with developing a theory of situated agency, for which Giddens appears to be the chosen theorist warranting engagement. It will be interesting to find out what direction any follow-up book will take.

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**John Laurent (ed.), *Evolutionary Economics and Human Nature*. Cheltenham, UK, and Northampton, MA, USA: Edward Elgar. 2003. xiii + 220 pp. ISBN 1 84064 923 2. £55.00.**

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**Steven Pressman\***

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This volume has few strengths and many weaknesses. A first weakness is its title. This is not really a book about evolutionary economics and human nature. Most chapters make only passing reference to evolutionary economics and do not provide an evolutionary account of human nature. With one exception, all are history of thought pieces covering how particular individuals have viewed human nature.

A second weakness concerns the figures covered – the ancient Greek philosophers (especially Plato and Aristotle), St Augustine, Adam Smith, Thomas Robert Malthus, Charles Darwin, Alfred Marshall, Peter Kropotkin, T.A. Jackson, Kenneth Boulding and Fritz Machlup. Inexplicably, there is no chapter on Veblen. Other notable exclusions are Rousseau, Kahneman and Tversky, and evolutionary game theory. I was equally baffled by some of the figures included in the volume. The chapter on the ancient Greeks correctly notes (p. 43) that ‘Aristotle’s own logic precludes an ingredient essential to any evolutionary account, that of change through time’. Plato was even less evolutionary in outlook than Aristotle. Similarly, the Boulding chapter questions whether he was really an evolutionary economist (p. 178), while Jackson does not seem to warrant an entire chapter. Finally, Machlup was an Austrian who saw individual rationality and selfishness as providing the foundation for understanding *individual behaviour*, although he did recognise that uncertainty pervades the world and that our *knowledge* of the world is evolving.

A third weakness is the quality of the individual chapters. Some do a good job of presenting particular individuals’ views on human nature. Others cover old ground or miss key points. Yet others do not belong in this volume (as noted above) or contain serious flaws.

Among the more worthwhile chapters are those by John Laurent on Darwin and Rob Knowles on Kropotkin. Laurent positions Darwin nicely in the history of economic thought. He shows how Smith’s *Theory of Moral Sentiments* influenced Darwin and led Darwin to believe in an altruistic impulse that evolved over time and that increased our chances for survival. Laurent then moves forward in history to show the influences of Darwin on Veblen and evolutionary game theory.

Knowles’s chapter is probably the best in book, in part because Kropotkin is typically ignored in the history of economic thought, despite the fact that his arguments on the advantages of mutual aid over selfishness have important economic implications. Like Laurent, Knowles shows that *The Theory of Moral Sentiments* influenced Darwin. He then shows how Darwin influenced the evolutionary social theory of Kropotkin. Kropotkin held that mutual aid could eliminate competition as a driving force in human life, and he provided substantial empirical evidence that many species evolve and prosper because individual members of the species provide aid and support to others.

Moving on to the problematic chapters, Athol Fitzgibbons on Smith argues that Smith did not see man as mainly selfish, and that *The Theory of Moral Sentiments* dealt with the tension between self-love and benevolence in all of us. A large number of scholars have recently made similar arguments, rejecting the greed-is-good misinterpretations of Smith (see Brown 1994; Rothschild 2001; Walsh 2000). Fitzgibbons, however, seems oblivious to this literature.

Peter Groenewegen argues that Alfred Marshall did not accept the rational economic man paradigm, but was evolutionary in his outlook and influenced by Darwin. He traces the notion of rational economic man back to John Stuart Mill's utilitarianism, and then discusses Marshall's use and rejection of this abstraction. Marshall accepted the rationality part of rational economic man, but not the selfish part, although he did recognise a selfish side to man. For Marshall, material progress and education could change us so that we would not always focus on our own gains.

While excellent on Marshall, Groenewegen does less well on Mill. Contra Groenewegen (p. 117), Mill's utilitarianism is a matter of considerable dispute. Nothing captures this better than Mill's (1957, p. 14) famous assertion in *Utilitarianism* that it is better to be Socrates dissatisfied than a pig satisfied. And, like Marshall, Mill recognised the importance of moulding people so that they do not focus exclusively on their own selfish interests. This has become a key issue in recent interpretations of *On Liberty* (see Pressman 2003).

Malthus is the subject of an exceedingly interesting, but ultimately flawed, chapter by John Pullen. While Smith thought that we want to truck and barter continuously, thereby consuming more, Malthus thought that people balanced the utility of consumption against the disutility of work. This is why he opposed the Poor Laws. Pullen argues that Malthus saw indolence as an *acquired* trait because he saw the Poor Laws as *encouraging* indolence (p. 87), and that this puts Malthus in the evolutionary camp rather than in the neoclassical camp, which sees indolence as a part of human nature and the result of rational calculation based on fixed constraints.

But Pullen moves much too fast here. Both an evolutionary Malthus and a neoclassical Malthus would see the Poor Laws as *encouraging* indolence, although for different reasons. To make the case for an evolutionary Malthus, the rich must differ from the rest of us for reasons other than the fact (as noted by F. Scott Fitzgerald) that they have more money than the rest of us. The case would go something like the following. Because Malthus did not worry about the indolence of wealthy landowners, and opposed taxing their wealth, he must have thought that they had different natures as well as more money. Thus, at bottom, Malthus must have thought that the wealthy had somehow evolved and developed habits that were good for the community, and for this reason we need not worry about their indolence from possessing too much wealth.

Finally, the great disappointments in this volume – Geoffrey Fishburn on the ancient Greeks, Laurent on St Augustine, and Jason Potts on evolutionary theory and human behaviour. Fishburn relies almost exclusively on the classic work of Arthur Lovejoy (1961) to interpret the ancient Greeks. The works of Plato and Aristotle fail to appear in his bibliography. Nor does Fishburn seem aware of the work by Martha Nussbaum (1986) on ancient Greek thought, and her contention that this work contains tensions between rational and non-rational aspects of human nature.

Laurent argues that St Augustine saw human beings as social by nature, that he saw society as a natural institution for preserving the social order, and that he anticipated Darwin because he saw society evolving in order to preserve order. Laurent, however, chooses much too selectively from St Augustine. The state and the social order for St Augustine were positive forces only when following the precepts of the Church and God. They were unjust and unnatural when people loved themselves more than they loved God and their fellow men. In this case, the state and society did not preserve order. When all is said and done, St Augustine did not think that man would evolve to accept religious truths and was not much of an evolutionary thinker. (For more on St Augustine, see Copleston 1962).

Finally, following Stephen Pinker (1997) and others, Potts sees the human mind as a complex program that has evolved over time. One key experience in the development of this program, according to Potts, was the nomadic existence of our African ancestors. When rains did not come, it was necessary to move; and humans adapted to their environment by moving. Potts argues that this nomadic instinct became part of the hardwiring that makes up the human mind, driving the growth of knowledge and the wealth of nations. Unfortunately, this is a truly awful example to use in attempting to illustrate the evolutionary aspects of the human mind. Nomads did not evolve in a cognitive sense; rather they moved based on rational calculations in a constrained environment. Those who could do the necessary calculations survived; those who could not did not survive. This foraging behaviour is consistent with neoclassical economics and supports neoclassical thinking; it does not and it cannot provide a foundation for evolutionary economics (see Field 2001, p. 307).

Without doubt, people must be brought back into economics. We need to recognise that our expectations, perceptions and preferences are affected by our environment, and also respond to it. There is good empirical evidence (from single-play prisoner's dilemmas, from acts of heroism, etc.) that people are not as selfish as economists believe them to be; and there is even good empirical evidence that, by studying economics, people evolve into more selfish and less altruistic individuals (Frank, Gilovich and Regan 1993, 1996). But, due to all its weaknesses, this book is not the place to find insights into these key issues. For those interested in how unselfish instincts could have evolved to become part of human nature, *Altruistically Inclined?* by Alexander Field (2001) and *Unto Others* by Elliot Sober and David Sloan Wilson (1998) are much better places to start.

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**Douglas Dowd (ed.). *Understanding Capitalism: Critical Analysis from Karl Marx to Amartya Sen*. London: Pluto Press. 2001. 183 pp. ISBN 0 7453 1782 0 (hb), 0 7453 1783 9 (pb). £50.00 (hb), £15.99 (pb).**

**Michael Schneider\***

Doug Dowd has an Australian connection, having spent a rest and recreation period in Australia during a stint with the United States Air Force in New Guinea in the Second World War. Does the new book he has edited live up to the expectations this connection may arouse?

*Understanding Capitalism* was avowedly 'written for those who, puzzled or upset by the functioning of the socioeconomy, need and want to have a surer grasp of capitalism's realities' than that to be obtained by reading the works of mainstream economists. In pursuit of this objective Dowd commissioned seven essays on writers (five) or schools of thought (two) critical of capitalism, prefacing them with an Introduction largely devoted to criticism of the description of capitalism provided by the neoclassical economic orthodoxy.

The focus of the essay on Marx by Michael A. Lebowitz is indicated by its sub-title, 'The Needs of Capital vs. The Needs of Human Beings'. For the 'puzzled', ignorant of Marx's view of capitalism, it provides a brief course. For those more familiar with Marx, however, it is short on new insights, and has some deficiencies. For example, there is an undiscussed tension between the essay's endorsement of Marx's view that capitalists continually and successfully strive to keep workers' wages at a minimum on the one hand, and its emphasis on 'the sales effort' (a term not used by Marx, as a footnote acknowledges) engaged in by non-consuming capitalists on the other – at whom is the sales effort directed? Inserting Baran and Sweezy into Marx muddies the waters, as Dowd seems to acknowledge in the second essay when he states that 'consumerism in Marx's time not only did not but *could* not exist'. Further, the pregnant observation that '[t]here is a long tradition in mainstream economics which proposes that it does not matter whether

capital hires labor or labor hires capital because the results will be the same in both cases' is not followed up by any exploration of what would happen if labour did hire capital; presumably the capitalists would receive a riskless return, and labour would reap the profits or incur the losses. The omission of references for quotations on the ground that they are to be found in Lebowitz's other works can be alternatively viewed as laziness.

Doug Dowd's essay on Veblen, while drawing some parallels between Marx's ideas and those of Veblen, hits the nail on the head in pointing out that in Veblen's view 'the average person *not* of the "leisure class" is more likely to seek to be like those who are, than, as Marx had hoped and expected, to seek a society in which the leisure class would be eliminated, through the workers' creation of an economic, political, and social democracy'. Towards the end of his comparison of Veblen's critique of the capitalism of his time with the approving description of it by mainstream economists, Dowd brings the discussion up to date by adding a footnote suggesting, with good reason, that '[w]hat is wrong with the form and content of mainstream economics, and alternatives to it, may be studied most beneficially in a recent book by Hugh Stretton, *Economics: A New Introduction*'. Given his emphasis on Veblen's role as a critic of mainstream economics, however, Dowd might have noted that the very term 'neoclassical economics' was invented by Veblen, in 1900, to describe the mainstream Marshallian economics of his day.

The third essay, written by Carl Boggs and entitled 'What Gramsci Means Today', unfortunately stands out like a sore thumb. Gramsci was not particularly interested in understanding capitalism – his writings were mainly directed to exploring how capitalism could be overturned, and this is the focus of Boggs's essay. It is worth noting, however, that Dowd's comment that 'Veblen's analyses of his status quo made very *good* sense, but it had nothing of *common* sense in it' has a counterpart in Boggs's multiple references to Gramsci's belief that a major obstacle to the overthrow of capitalism is the success of the ruling class in persuading workers that the power, wealth and status characteristics of a capitalist society are natural, no more than common sense.

Michael Keaney's essay entitled 'Critical Institutionalism: From American Exceptionalism to International Relevance' also stands out, but as a comprehensive (except as noted in the next sentence) and incisive account of how capitalism has been viewed by institutionalists from Richard T. Ely to Marc Tool. Excluded from discussion is the work of the 'new institutionalists', on the ground that 'in defining institutional efficiency as the minimization of transaction costs, the "new institutionalists" are simply applying neoclassical principles to another aspect of social life', and their derisory term 'old institutionalists' is rejected in favour of 'critical institutionalists'. Unlike mainstream economists and new institutionalists, critical insitutionalists are claimed to have been able to foresee the catastrophe which occurred in the former Soviet Union when the 'shock therapy' of simply inserting core capitalist institutions was applied. Among the critical insitutionalists, however, Clarence E. Ayres is criticised for his thesis that 'development' resulting from the introduction of European technology 'was most pronounced where cultural resistance was least. Some readers may find that euphemistic in the extreme, especially as applied to North America and Australia'. While noting that 'John A. Hobson' (sic) was '[p]erhaps the earliest European economist to take more than a passing interest in institutionalism', Keaney rightly concludes that 'Hobson was too idiosyncratic to be easily pigeonholed as an institutionalist'. Also discussed are Gunnar Myrdal's ideas, including not only those to be found in his best-known

work, *An American Dilemma* (1944), but also those to be found in the equally important *The Political Element in the Development of Economic Theory* (1953), where '[r]ejecting the possibility of value-neutrality, and highlighting the metaphysical and teleological assumptions underpinning neoclassical economics, Myrdal argued that values were central to any purposeful inquiry.'

Post Keynesian economics, like institutionalism, is a house divided against itself, as Fred Lee acknowledges in 'Post Keynesian Economics (1930-2000): An Emerging Heterodox Economic Theory of Capitalism', where the 'liberal' (capitalism's shortcomings can be ameliorated) branch of Post Keynesian economics is distinguished from the 'radical' ('capitalism cannot be made to work better, rather it must be changed') branch favoured by Lee. His essay is divided into two parts. The first part covers works critical of the neoclassical description of capitalism written during the period 1930 to 1970; though the earliest work mentioned is an unidentified piece by Gardner Means written (published?) in 1935, 1930 is chosen as the starting date because of the importance of the Great Depression in inspiring such criticism. The second part begins by tracing the history of the establishment of the Post Keynesian paradigm, from the meeting organised by Alfred Eichner and attended by Joan Robinson and sixteen 'sympathetic' economists at the American Economic Association meeting held in 1971, to the numerous associations and journals which made up the Post Keynesian network thirty years later. There follows an attempt, quixotic perhaps, to explain what Post Keynesian theory is, an attempt which gets off to an unfortunate start when after stating that 'a capitalist economy can be pictured or represented in terms of two overlapping inter-dependencies', Lee goes on to list three, namely those between outputs and inputs, incomes and expenditures, and money and goods and services. Even when we add Lee's accounts of Post Keynesian approaches to these commonplace inter-dependencies, it seems clear that after thirty years the heterodox Post Keynesian paradigm is still to 'emerge'.

Among the precursors of Post Keynesian theory mentioned by Lee is Paul Sweezy, the subject of John Bellamy Foster's essay entitled 'Paul Sweezy and Monopoly Capital'. Foster points out that while by the 1930s Marxian economists had linked the theory of monopoly capital with that of imperialism, they had not yet linked it with the theory of capital accumulation and crisis. Kalecki and Steindl made a start, but it was Sweezy, in conjunction with Baran, who in *Monopoly Capital* (1966) linked industrial concentration with Marx's theory of realisation crises. In a final section Foster provides a stimulating update of Sweezy's analysis, including a prescient reference to 'aggressive US attempts to *preempt* ... challenges by extending the geopolitical sphere of its hegemony' (italics added), and arguing that contrary to a common view on globalisation, national sovereignty in advanced capitalist countries (by contrast with 'the periphery') has not been eroded.

'Amartya Sen: The Late Twentieth Century's Greatest Political Economist?' is the title of the last essay, written by Robin Hahnel. The sub-title reflects the admiration Hahnel has for Sen's numerous criticisms of neoclassical economics, notably the latter's unwillingness or inability to deal with questions of distributive justice, and the question mark reflects his disappointment that he has found 'Sen's critical work more intellectually compelling than his positive work'. It is ironic, Hahnel adds, that nonetheless Sen's critique of neoclassical economics has had a largely unreceptive audience, while his new approaches to the analysis of poverty and development have found an appreciative audience in critics of institutions such as the IMF and the World Bank. Under the heading 'High Theory'

Hahnel provides a good, though unavoidably rather technical, account of Sen's critical work, though he might have noted that Sen decided as early as 1973 that making interpersonal comparisons cannot be avoided if distributional questions are to be dealt with. There follows an extensive critique of Sen's critique, arguing in particular that while rightly discarding the theory of individual rational choice as a tool for prediction, Sen should have retained it as a tool for evaluating core economic institutions. Under the heading 'Brutal Reality' Hahnel provides a largely appreciative summary of Sen's more recent writings on famine, poverty and human development, though he objects to what he sees as Sen's reduction of the capabilities that people may aspire to as mere expansions of freedom.

In sum, while this book contains much of interest to the heterodox economist, it includes at least five conflicting views of 'capitalism's realities', a handicap if its purpose is to give the 'puzzled' a 'surer grasp' of these realities.

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<p><b>Brian Snowdon. <i>Conversations on Growth, Stability and Trade: An Historical Perspective</i>. Cheltenham, UK, and Northampton, MA, USA: Edward Elgar. 2002. xvi + 483 pp. ISBN 1 84064 995 X. £75.00</b></p>
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**John Singleton\***

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Brian Snowdon states in the preface that one of his aims in recent years has been to make economics more interesting by incorporating interviews with leading practitioners into textbook surveys. In *Conversations on Growth, Stability and Trade* he succeeds with flying colours. The first half of the book comprises five chapters, which discuss the development of economists' thinking on the evolution of the international economy, economic growth and development, macroeconomic instability and its remedies, and international economic integration. Snowdon provides an excellent survey of the literature, at a level that ought to be accessible to third-year undergraduates. (Colleagues to whom I have shown the book concur.) My only quibble with this part of the book is that he sometimes gets carried away with enthusiasm, peppering the text with exclamation marks. But I'm glad that I resisted the initial temptation to skip these chapters and move on to the interviews themselves.

The second half of the book consists of substantive interviews with the following economists: Ben Bernanke, Jagdish Bhagwati, Alan Blinder, Nick Crafts, Bradford DeLong, Barry Eichengreen, Kevin Hoover, Charles Jones, Christina Romer and Joseph Stiglitz. Clearly, Snowdon has not chosen a purely random sample from the population of well-known economists with interests in growth, stability and trade. He has, rather, chosen economists who are in the main sympathetic to the Keynesian tradition, and believe that economic history and institutions matter. All are Americans, except Bhagwati, though even he is based in

the USA, and Crafts who is British and teaches at LSE. While some of the interviewees may be considered as elder statesmen, DeLong, Hoover, Jones and Romer represent a somewhat younger cohort. The content of the interviews is fascinating. Snowden is well-informed and asks perceptive questions. The interviewees are enthusiastic about their research projects and, in the main, express themselves clearly. The discussions are wide-ranging, covering methodology, the relevance of economic history, the debate between endogenous and neoclassical growth theories, the Great Depression of the 1930s, the great inflation of the 1970s, trade policy, population growth, globalisation, the Asian miracle, the Asian crisis, inflation targeting, the 'new' economy of the 1990s, the prospects for convergence between rich and poor nations, and so on.

The main theme running through the interviews is the revival, in the 1980s and 1990s, of economists' interest in the causes of long-term growth. In a sense the promoters of endogenous growth theory have reinvented the wheel – indeed Stiglitz hints at this when commenting that Schumpeter was an endogenous growth theorist. Nevertheless, it was useful for the discipline to break out of the previous theoretical straitjacket. What is the role of economic history in modern growth economics? History can always be ransacked for data, although most are of highly dubious quality. Equally importantly, history provides knowledge about institutions and about how they evolve, which varies considerably from country to country. The Russian disaster of the 1990s illustrates the danger of imposing a simplistic free market blueprint on an economy with no recent experience of such arrangements. The economists represented in this volume are sensitive to these complications. To paraphrase Eichengreen, in contrast to many of their colleagues the best economists do not allow method to crowd out substance. Eichengreen and Crafts are cautiously optimistic about economic history's ability to contribute to the debate, though Crafts laments the fact that many economic historians are still unwilling and/or unable to discuss long-term economic change in language that means anything to economists.

There are blemishes. Several important issues are neglected. For example, what is the impact of long-term growth on the environment? Also, once people have attained a decent standard of living and life expectancy (equivalent to Australia and New Zealand in the 1950s) does further growth make them any happier?

This book is an invaluable guide to the thinking of leading economists and economic historians. Snowden should continue the good work.

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